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1 MORNING SESSION, FRIDAY, JULY 7, 2006

2 DR. COURTNEY: Okay. So let's kick off
3 the morning. Here's what we'd like to do for the
4 morning. We've done a fair amount of work overnight and
5 talking to a number of -- the panel talking amongst
6 themselves, and we actually feel we're making some good
7 progress. We're happy with yesterday's meeting, we
8 learned a lot. Actually feel like we've been able to,
9 like I said, understand what the issues are, maybe even
10 start to understand, perhaps, why there have been some
11 disagreements amongst the studies, and maybe see some
12 resolution for that.

13 This morning we have a couple of folks
14 calling in. Here's what we'd like to do. Dr. Vignieri
15 is going to call in in 10 minutes' time and come on the
16 conference line. We're going to talk with her briefly.
17 We actually feel like we understand the points, but
18 there are comments by email and critique; so that may be
19 a brief conversation, depending on what she would like
20 to bring to the meeting.

21 Then we're going to switch over to
22 talking about, perhaps, one of the main philosophical
23 issues, which is what the heck is a subspecies, talking
24 about how to define those things, how people have
25 defined subspecies, and that'll be primarily beginning

1 with at least a conversation started off by the panel
2 who are going to talk about potential definitions,
3 things people have said, alternatives that are out there
4 in the literature. We'll talk a little bit about the
5 definition of subspecies, and then it'll become a
6 conversation with the scientists present and the panel
7 about what's the functional definition of subspecies,
8 how would you apply it to research.

9 Then at 10:30 our time, Jim Patton from
10 U.S. Berkeley is going to call in. We asked him to call
11 in at that point because that's actually the issue he's
12 going to be addressing, which is again what constitutes
13 subspecies in species of small mammals.

14 And then after his phone call, we're
15 going to stop. We're not going to have another session
16 at that point. We're going to take the panel away and
17 think about what we want to do at that point. So it
18 doesn't mean we're not going to come back. It doesn't
19 mean we're not going to listen to anything else you want
20 to bring to the table. It just means we're going to go
21 away and think and come up with what we want to do with
22 the rest of the day.

23 So we may stop at 11:00 and take a long
24 break and come back after lunch. We may stop and come
25 back -- and come back at noon. I think the panel just

1 has to figure out what they want to do at that point.

2 So I'm just giving you heads-up that this morning is
3 essentially listening to folks call in, and then direct
4 from the panel, and the discussion starting with
5 Dr. Vignieri's phone call about what is a subspecies.

6 So anything else that, you know, needs to
7 be discussed or brought to the panel, well, you know,
8 we're ready to listen to things that you feel need to be
9 addressed. Same old system, bring your comments to me,
10 and I'll certainly take note of them and bring them to
11 the panel as necessary, and we'll bring you back as the
12 panel will see fit. But we're actually feeling fairly
13 good about the process, feel that we've made some
14 progress, so we have every hope of actually getting
15 through some of this material fairly rapidly.

16 So with that, we've got 7 or 8 minutes
17 before session comes on line. Is there anything the
18 panel wants to say at this point?

19 DR. DUMBACHER: Not really except I had a
20 chance to talk to some of the people who participated
21 yesterday, and I just wanted to thank you all very much
22 because I know it was -- I mean, I hate for my own
23 research to have to go through that kind of scrutiny.
24 And we really appreciate your honesty and
25 forthrightness; and it's been extremely helpful to us in

1 DR. DUMBACHER: Good morning,
2 Dr. Vignieri.

3 DR. VIGNIERI: Good morning.

4 DR. DUMBACHER: This is Jack Dumbacher.

5 DR. VIGNIERI: Hi.

6 DR. STEPPAN: Scott Steppan.

7 DR. VIGNIERI: Hi.

8 DR. ARBOGAST: We thought what we would
9 do, we have received the documents that you sent
10 regarding the studies of Ramey, et al.; and King,
11 et al., and thank you for providing those. And while
12 the documents are fairly straightforward, what we
13 thought we would do is ask a few specific questions and
14 then allow you to make any substantive points at the end
15 that have not been already covered in the written
16 documents.

17 DR. VIGNIERI: Okay.

18 DR. ARBOGAST: So I don't think we need
19 to rehash things that were clear in those, but if you
20 have additional things, then that would be the time to
21 add those.

22 DR. VIGNIERI: Okay.

23 DR. ARBOGAST: Okay. Great. So the
24 first question that we wanted to ask is you argue that
25 the original characters used in the Krutzch diagnosis of

1 Preble's mouse were not tested by the morphological
2 analyses of Ramey, et al. There's been some discussions
3 about this, and what we would like is to get your take
4 on -- your position on this and specifically about which
5 characters were and were not tested and what effect that
6 has.

7 DR. VIGNIERI: Okay. So I knew you guys
8 were going to ask me this question, and I am not the
9 morphology expert on our paper. Brad Bergstrom has
10 submitted, as of this morning, official comments on the
11 morphology. I can say that, from my knowledge, we know
12 that Ramey, et al., did not test all of the
13 morphological characteristics that were used in the
14 description and some of which they called qualitative,
15 but we -- we differ in that opinion.

16 So we feel that they didn't specifically
17 test the hypothesis, rather they just took the nine
18 standard measurements that were conjured up during those
19 times and used those to try and detect a difference and
20 rather than testing Krutzch's actual hypothesis using
21 the same traits that he used.

22 And like I said, this is not my area of
23 expertise, and I refer you to Brad Bergstrom for more
24 details on that as well as Jim Patton who will be
25 calling later because he addresses that as well.

1 DR. ARBOGAST: Okay. Thank you. Do you
2 guys have any follow-up questions on that?

3 DR. DUMBACHER: That's probably good. We
4 will be talking to Jim Patton hopefully later this
5 morning, so we can talk to him about those things as
6 well. So thanks very much.

7 DR. VIGNIERI: He is certainly the expert
8 on that relative to me, so . . .

9 DR. ARBOGAST: Wonderful. Thank you.
10 The next question is: You also commented that you
11 believe that characterizing the *Zapus hudsonius* as a
12 habitat generalist is incorrect. Could you please
13 expand on why you believe that and why that distinction
14 may be important in the case of Preble's jumping mouse?

15 DR. VIGNIERI: I believe that because, in
16 general, in the data stages, there's some pretty strong
17 affiliations with riparian and muroid habitats; and I
18 think that, basically, negates the argument that they're
19 generalists. They're clearly specialized in riparian
20 habitats, and that's been noted in all the citations
21 that were mentioned by Ramey, et al., in their paper.

22 So they clearly, while they may not
23 specialize on a certain plant type or fungus or other
24 type of foliage, they certainly do specialize in
25 riparian habitat. So I think that's pretty well-known

1 generalists really. I mean, there's been quite a bit of
2 research done in this genus and they tend to not really
3 be generalists in terms of their diet. They eat a wide
4 variety of foods. But, like, for example, it's
5 well-known that they need to have specific green seeds
6 and foliage at the end of summer in order to fatten up
7 enough for hibernation.

8 So again, they're not like *Peromyscus* out
9 there, really diversified -- or using a diverse array of
10 foliage. They've been known to eat a variety of plants,
11 but generally the plants have been affiliated with
12 water. They've been known to eat some fungus, some
13 seeds, some fruits, and fish. So you know, there's
14 certainly not a single food that they specialize on, but
15 they're not eating everything out there.

16 DR. ARBOGAST: Okay. Great. And then
17 the last question I had was you also argue -- and this
18 -- we touched on this just now -- but you also argued
19 that the ecological exchangeability criterion has not
20 been adequately tested. How -- how do you think this
21 could be tested in the case of *Preble's*?

22 DR. VIGNIERI: Well, I mean, there are
23 certainly sort of classic ways of actually doing
24 exchanges. I think that might be difficult in the
25 species given that it is listed, but there are other

1 ways to estimate whether or not species are ecologically
2 exchangeable just by studying the more basic ecology of
3 each of these species. We really don't know that much
4 about these subspecies in terms of even their specific
5 diet or, you know, their detailed ecology.

6 And just some basic ecology studies, I
7 think, on these -- the species in question would really
8 help us to understand better whether or not they are
9 exchangeable. I think a true test -- you know, you have
10 to sort of really look at how these two subspecies might
11 do in each other's habitats, but a proxy for that at
12 least would be to understand the ecology.

13 And in Ramey, et al., they cited a few
14 reviews as their evidence for these species not being
15 ecologically exchangeable, but the triad of those
16 citations were -- I mention this in the documents, so I
17 won't go into it, but basically the studies just haven't
18 been done, and the most -- the most thorough of the
19 reviews does clearly say that there's a lack of
20 ecological research in the subspecies. So if we can get
21 some of that information, that would really go a long
22 way for us understanding whether or not there is true
23 ecological divergence.

24 DR. ARBOGAST: Okay. Thank you. Do you
25 guys have follow-up questions?

1 DR. STEPPAN: Yeah, I had a question.

2 This is Scott Steppan, can you hear me all right?

3 DR. VIGNIERI: Yes.

4 DR. STEPPAN: Okay. So have you done
5 work on dispersal abilities or dispersal patterns in
6 *Zapus*?

7 DR. VIGNIERI: I have, yes.

8 DR. STEPPAN: Do you have any insights
9 that might be relevant regarding the possible
10 dispersible acts over short time frames or long time
11 frames?

12 DR. VIGNIERI: Yeah. So I worked in
13 *Zapus trinotatus*, and I looked at -- specifically at
14 dispersal and also at how the distribution of specific
15 habitat, in this case, riparian habitat affects
16 dispersal. And I found that dispersal is definitely
17 restricted in the species by the location and
18 distribution of riparian habitat and this did result in
19 pretty strong spacial structure over short distances.

20 So basically I found significant spacial
21 structure within, you know, 10 kilometers among
22 populations and these dispersals were found to be
23 following riparian pathways, so I think there is a real
24 limitation placed on dispersers in this -- in *Zapus*
25 *trinotatus*, and by association, I would expect somewhat

1 in *Zapus hudsonius* as well.

2 And also looking at sort of more standard
3 marker capture data, I measured dispersal distances and
4 they're very short in these species, you know, between,
5 60 and 150 meters generally. So they're not -- they're
6 not moving very far, and when they are moving over time,
7 they're restricted in their movement. And I do think
8 that you would expect to see divergence in this rodent
9 more so than you would in other rodents because of these
10 limited dispersal capabilities and also this habitat
11 affiliation.

12 DR. DUMBACHER: So this is Jack
13 Dumbacher. So the dispersal that you measured is natal
14 dispersal; is that correct?

15 DR. VIGNIERI: Right.

16 DR. DUMBACHER: And this is a demographic
17 measure of dispersal?

18 DR. VIGNIERI: Yeah. So I measured natal
19 dispersal, and then I've also measured sort of more
20 long-term migration, as we would think of it, among
21 populations. And the natal dispersal is what I can
22 actually measure and quantify and say, you know, they're
23 only moving 150 meters; but in terms of migration and
24 long-term gene flow, that's where I also found that they
25 appear to be limited in their movement by this habitat

1 distribution.

2 DR. STEPPAN: Have you -- this is Scott
3 Steppan again. Have you looked at any aspects dealing
4 with habitat change over the last, you know, hundreds or
5 few hundreds of thousands of years in terms of how the
6 patchy habitat itself may have shifted between some of
7 these regions?

8 DR. VIGNIERI: You know, I can't say that
9 I've looked at that specifically. Certainly -- yeah.
10 No, I mean, the studies that I did weren't more of the
11 sort of long-term phylogeographic studies, so I don't
12 have the data to look at those kind of large habitat
13 changes. I really did most of my work in a relatively
14 small area, looking at fine scale spacial joint
15 structure, so --

16 DR. STEPPAN: Okay.

17 DR. VIGNIERI: -- I guess say I can't.
18 But I could make some guesses about that; but I mean,
19 basically no. In terms of -- if I do address and sort
20 of try to expand from that work that I did do, I did
21 sort of look at how past migration patterns -- so if we
22 look at the more standard ecological markers like F_{st} , we
23 think of those as looking at historical patterns, and
24 I'll also compare those with more recent migration rate
25 estimators, so within the last two generations that

1 found consistency between those.

2 So I do believe that the patterns that I
3 saw were not a result of some changes in the habitat and
4 then that being carried over to what we see today. I do
5 believe that half of the riparian habitat now is
6 restricting these species as I believe it was in the
7 past.

8 DR. STEPPAN: Okay.

9 DR. ARBOGAST: This is Brian again. What
10 about in the sex-biased differences in dispersal?

11 DR. VIGNIERI: So what I found was that
12 there is a -- there is a small sex -- there is sex bias
13 in dispersal. Males are the dispersers, but they don't
14 go very far, so really the difference in distances is
15 minimal. Over time, you know, that does even out to
16 some degree, but I didn't find huge differences, say,
17 where, you know, males are going hundreds and hundreds
18 of meters and females are not moving at all. So I
19 believe that largely males are the main dispersers, but
20 I think that females also move to some degree as well.

21 DR. ARBOGAST: And I have one more
22 question. So given -- given your experience and the
23 fact that if we do assume that the movements were also
24 small, the dispersal movements in *Zapus hudsonius*, how
25 is that relevant to or is it relevant to any of the

1 results that have been presented in any of the studies
2 that we've reviewed?

3 DR. VIGNIERI: Well, I think it's related
4 to the genetic structure that we've seen among these
5 species. And also if you look at the King, et al., they
6 also found genetic structure within subspecies. And I
7 think that this -- this limited dispersal both,
8 obviously, just by physical abilities and also by
9 habitat limitations has -- has generated these
10 patterns -- these stronger patterns of genetic structure
11 that you see both within the subspecific level and also
12 between subspecies.

13 As I said, I think you get stronger
14 genetic structuring more rapidly, so greater divergence
15 over a greater -- or over a shorter period of time
16 because of these habitat limitations placed on
17 dispersers than you would in rodents that are truly
18 generalists.

19 DR. ARBOGAST: Okay. Thank. Do you guys
20 have anything else? Okay. Well, thank you. And I'll
21 just end by allowing you to add anything that you think
22 would go beyond what you provided in the written form or
23 just now during this meeting that would -- that you feel
24 is especially substantive.

25 DR. VIGNIERI: Okay. The one thing I

1 wanted to just point out -- because I haven't been
2 there, I'm not sure, you know, whether you guys have
3 discussed this -- but I think -- I think this question
4 of reciprocal monophyly is something that really should
5 be dropped from the debate, because as we did point out,
6 it's somewhat inappropriate at the level of subspecies.
7 And if we look at the results of King, et al., and then
8 also as we said in our documents that we believe the
9 more preliminary results of Ramey, et al., you do see
10 that there's strong evidence for genetic divergence.

11 And if we look at, like, Moritz, for
12 example, which many people keep using -- citing him as
13 being sort of the be-all, end-all for defining what an
14 evolutionary or a significant unit is, he basically
15 states that you might expect to have reciprocal
16 monophyly at the level of an ESU, which he considers at
17 the level of a species; and again, we're looking at
18 subspecies level, which he states are more likely the
19 level of management units.

20 And we -- we definitely, in both studies,
21 meet the criteria for these subspecies level divergence
22 that Moritz points out. They were very significant
23 divergence at little frequencies. We have no showing of
24 haplotypes; strong Fists, which do give an indication
25 that some mutational changes have occurred, which is

1 even greater evidence that the divergence we're seeing
2 is not just genetic drift.

3 So I just wanted to point out that I
4 think this kind of focus on reciprocal monophyly is just
5 completely inappropriate in the debate and that if we
6 look at the data that we have in both studies, it's
7 pretty clear that these subspecies are meeting the
8 requirements for significant genetic divergence as
9 pointed out by some of the authors that are actually
10 cited as the -- sort of people we're trying to meet the
11 requirements of, so . . .

12 DR. ARBOGAST: This is Brian again. So
13 did Moritz specifically equate an ESU with the species
14 or is that --

15 DR. VIGNIERI: He does state -- I was
16 just actually reading this. He does state that we would
17 see that an ESU level would be more -- more likely to be
18 seen at species. I wish -- I could try to see if I have
19 that still up here.

20 DR. ARBOGAST: I'm sure we can have
21 access to that. I was just curious.

22 DR. VIGNIERI: Yeah. I just want to make
23 sure that I gave you guys the right paper. I believe
24 it's the Tree paper that he states that.

25 DR. ARBOGAST: Wonderful.

1 DR. COURTNEY: We actually have Moritz'
2 paper here.

3 DR. VIGNIERI: Good.

4 DR. ARBOGAST: Okay. Thank you.

5 DR. VIGNIERI: So that's -- that's all --
6 I mean, I have a lot of other things I could say, but it
7 sounds like you guys are progressing well and it's sort
8 of difficult doing this over the phone, so . . .

9 DR. ARBOGAST: I do think we have a lot
10 of it in written form, so that's very helpful. Okay.
11 Well, thank you very much.

12 DR. COURTNEY: Before you go Sacha, I
13 feel it's incumbent, since everybody else who has been
14 part of this process, you know, we've had the
15 opportunity for anybody to pass up questions to be
16 addressed if there's any -- does anybody have things
17 that they want us to be addressing with Dr. Vignieri?
18 I'm seeing blank stares, so if that's cool and the panel
19 have nothing more.

20 DR. ARBOGAST: That's all.

21 DR. DUMBACHER: That's all.

22 DR. COURTNEY: Actually hang on one
23 second.

24 DR. RAMEY: I was going to ask -- Sacha,
25 it's Rob Ramey over here. Hello.

1 DR. VIGNIERI: Hi.

2 DR. RAMEY: I was going to ask what would
3 be your critical test of ecological exchangeability
4 utilizing some ecological study that could be done
5 beyond what has already been done over the last hundred
6 years?

7 DR. VIGNIERI: Well, I think --

8 DR. RAMEY: And what was -- specifically,
9 what will be the critical test?

10 DR. VIGNIERI: I think the question -- I
11 want to point out that there really has not been
12 ecological studies -- many ecological studies done on
13 these species -- on this particular -- I mean, any.
14 There has not been any studies done on this particular
15 question. And there really has been very few studies
16 done that look at, say, comparisons between the
17 potential subspecies.

18 And I think at the very least, if you
19 want to ask questions about ecological exchangeability,
20 you have to be looking at similar things, similar
21 components of the ecology in the two species you're
22 proposing are ecologically exchangeable. So most of the
23 studies done on this species have been more descriptive
24 and those types of studies are likely to miss the kind
25 of detailed sort of questions about ecological

1 exchangeability that would really be required to address
2 that question. So even though you say in the last
3 hundred years, there's really very few studies that have
4 been done.

5 We say in our paper that there's, you
6 know, only been six citations in the last 40 years. So
7 we're not looking at species that have been,
8 particularly at the subspecies level, well studied and
9 whose ecology is well-known.

10 So at the very least, if you want to know
11 more about ecological exchangeability, I'd say that in
12 the three subspecies that are being proposed for
13 synonymy, you basically look at some ecological
14 questions such as, you know, diet and movement and
15 habitat use. And you look at those in the same way in
16 the three studies and you just -- in the three
17 subspecies and just try to determine if there's really
18 differences.

19 DR. RAMEY: Thank you, Sacha.

20 DR. DUMBACHER: Any other questions?

21 DR. ARBOGAST: Thank you very much for
22 your time. We really appreciate it.

23 DR. VIGNIERI: Thank you.

24 DR. COURTNEY: And thank you, Sacha.

25 This is Steven Courtney, thank you for making such an

1 effort. We really appreciate all the work you've done
2 to try to make sure that your group was well
3 represented, and I personally give you my thanks for all
4 your effort.

5 DR. VIGNIERI: Well, thank you. It's
6 been nice to be able to participate, and I wish you guys
7 the best of luck for the rest of the meeting.

8 PANEL MEMBERS: Thank you.

9 DR. VIGNIERI: Thank you. Bye-bye.

10 DR. COURTNEY: Okay. So the next thing
11 we're going to do in terms of where we're headed is to
12 talk a little bit about subspecies concepts, and I think
13 the panel are going to lead that discussion; is that
14 correct?

15 DR. DUMBACHER: Yes. Let me just call a
16 couple things up here real quickly. So I guess I'll
17 kick off the discussion here, and I think that this will
18 be more of a discussion than some of the other things
19 that we've been doing, but one of the cruxes here is
20 what is a subspecies and so we've tried to get a couple
21 of different definitions here that have been used in the
22 literature.

23 So going back to the time and place when these
24 original descriptions were first written and when
25 Krutzch was examining them, the paradigm was that Ernst

1 Mayr had a new biological species concept; and if I
2 remember it correctly, preblei was originally described
3 as a species and then it was --

4 DR. STEPPAN: No, it was a subspecies.

5 DR. DUMBACHER: So in 1899 it was first
6 described as a subspecies?

7 DR. STEPPAN: I take that back. I'll
8 retract that comment.

9 DR. DUMBACHER: Okay.

10 DR. STEPPAN: Preblei was first named in
11 '53, correct, by --

12 DR. RAMEY: '54.

13 DR. STEPPAN: '54 by Krutzsch as a
14 subspecies.

15 DR. DUMBACHER: Okay. And at the time,
16 subspecies were thought of -- and here's a definition
17 from Rentzsch and then another one from Mayr that are
18 similar. And in Rentzsch's work, "A subspecies is a
19 complex of interbreeding in completely fertile
20 individuals, which are morphologically identical or vary
21 only within the limits of individual ecological and
22 seasonal variability. The typical characters of this
23 group of individuals are genetically fixed and no other
24 geographical range as the same species occurs within the
25 same range.

1 And here's another definition from Mayr's
2 that's a little bit more easily digested. "Subspecies
3 are a geographically localized subdivision of the
4 species, which differs genetically and taxonomically
5 from other subdivisions of the species."

6 And Mayr and Ashlock have another
7 definition in '91, which is a little bit updated, but
8 again it's the same authors and the same basic species
9 definition that they're working with, which my
10 understanding is the operational definition that's used
11 in mammalian taxon is the biological species concept.
12 And I know that there's some debate about that, but I
13 don't know -- we'll talk about that. It looks like --

14 DR. STEPPAN: I would say -- do you want to
15 restate that?

16 DR. DUMBACHER: No. Actually -- I mean --
17 I guess, actually, in the avian world, which is what I
18 know best because we're talking about mammals here --
19 this will be a totally free-form discussion here because
20 this is something that we haven't sat down and talked
21 about. But in the avian world, we still do recognize --
22 I should say that the ALU, as far as I know, still --
23 well, I shouldn't even say that.

24 There's a lot of debate about what a
25 species is and what a subspecies is, so let's just leave

1 it at that for now.

2 DR. STEPPAN: Just to follow on that
3 thought, it does strike me that the bird/avian community
4 still largely follows, although I don't know by edict if
5 there is such a thing, a biological species concept.
6 Because there have been, I guess, suggestions to apply a
7 biogenetic species concept to birds, which would roughly
8 double the number of birds' species. And those
9 proposals have been made, but it doesn't seem like it's
10 been followed through in a generalized way.

11 Within the mammal community, I'll just
12 express a personal opinion because I think that's all I
13 can do in this circumstance, which is that there is no
14 governing body of how to define species within the
15 avian community, and I would say the majority of workers
16 largely have followed a biological species concept.

17 DR. ARBOGAST: I think that, in general,
18 there's a nomenclature committee, right, I mean, like
19 the Society of Mammalogy? And what they do is
20 occasionally review the literature to see if any groups,
21 you know, would deserve to be, you know, changed. But I
22 think that's just like a panel expert opinion of whether
23 they think it should be changed or not.

24 DR. STEPPAN: But that's using -- dealing
25 with taxonomy as opposed to issues of species, applying

1 species concepts, correct?

2 DR. ARBOGAST: Right.

3 DR. DUMBACHER: But I think that a lot of
4 us here on the panel would -- might agree, I don't know.
5 We'll see if we do all agree.

6 Let me just speak for myself and say that
7 within the current framework of taxonomy in the bird
8 community, which follows the biological species concept,
9 there are these recognizable entities below the species
10 level and the -- we don't have any sort of agreement
11 about what the definition for those things should be,
12 which is part of the problem here; but if we would
13 follow Mayr and -- Mayr's definition, the key components
14 here -- and I've seen a couple of these things in
15 various papers in print -- is that there are genetic
16 differences among these subspecies. There are usually
17 range or habitat differences, so they are usually
18 geographically isolated and they're geographically
19 centered; so there are geographical distances.

20 They must be diagnosable, so if we can't
21 tell them apart somehow genetically, morphologically, or
22 using some sort of characters, that makes it sort of
23 hard to define a subspecies.

24 But what's key is that they're not
25 reproductively isolated. If we all agree that they were

1 completely reproductively isolated and there's no gene
2 flow, then we would call those different species. So
3 what we're dealing with at the subspecies level is that
4 there is some gene flow, and that's -- these are the
5 definitions that I would say are mostly -- I should say
6 were applied at the time when these species were first
7 described and included in the literature, which maybe
8 see what the panel members think of that statement, and
9 correct me.

10 DR. STEPPAN: I mean, I would agree with
11 that. I would actually add a few minor comments on how
12 to interpret that. One of the points is that definition
13 has -- the one I'm working from '91 -- a collection of
14 populations that are diagnosably distinct, and so it's
15 the populations which are diagnosable as opposed to
16 individuals. And so that distinction may be relevant,
17 for example, comparing this to a phylogenetic species
18 concept as articulated by Cracraft, which is that
19 species -- species are diagnosably distinct units which
20 have a history of parental ancestry, parent/descendent
21 relationships.

22 And so in many cases, what -- Mayr and
23 Ashlock's subspecies would be considered to be species
24 under a phylogenetic species concept with the possible
25 distinction of whether Mayr and Ashlock are talking

1 about diagnosing populations, which is a less stringent
2 requirement and diagnosability than diagnosing
3 individuals explicitly.

4 And so that allowance is that, I think,
5 collection of populations might be really critical in
6 allowing certain amount of gene flow; that you have
7 individuals that may -- in some cases, one population
8 may resemble other individuals in another population or
9 another subspecies more, but that if they're part of the
10 local deme and that is a set of populations, they're
11 nonetheless fairly distinguishable, that it would still
12 satisfy their criteria.

13 DR. ARBOGAST: I think that's a good
14 point. I haven't really made that distinction.

15 DR. STEPPAN: What makes a population
16 diagnosably distinct is still a little unclear. Whether
17 it is simply the ability to detect statistically
18 significant differences and means for some particular
19 trait, that can be done, but that doesn't make them
20 easily diagnosable. That may be distinguishable, but
21 not diagnosable.

22 DR. DUMBACHER: So if I might move to the
23 next step. So one of the problems with these
24 definitions is they're not necessarily easily applied or
25 operational. And when I sat on the spotted owl panel,

1 we were stuck with this dilemma, and we scoured the
2 literature to try to find some sort of an operational,
3 easily applied definition and we found a couple. And
4 I'm not necessarily saying that these are -- that these
5 are good ideas, and I'm not saying that the biological
6 community would all agree this is the way it should be
7 done; in fact, I would say we probably wouldn't. But it
8 was a definition that we were able to apply in that
9 case, and I'm just going to read it to you.

10 And like I said, I'm not going to say
11 that this is what we are going to do, and I'm not saying
12 that this is what we should do, and I'm not saying the
13 biological community would agree with us; but we were
14 able to find a definition from Amidon in 1949. And
15 because of this problem with being able to diagnose
16 populations but not necessarily correctly assigning all
17 individuals, they derived a 75 percent rule for
18 delineation of subspecies in which 75 percent of a
19 population must be distinct or diagnosably different
20 from 75 percent of the individuals of the other
21 population.

22 In another paper by Patton and Union in
23 2002, formalizes even further and provided a
24 quantitative method for determining the validity of
25 subspecies. And under their methods, to be a valid

1 subspecies, 75 percent of the population must lie
2 outside 99 percent of the range of the other population
3 for a given defining character or set of characters.

4 Okay. So this all seems fairly dogmatic,
5 and I can say that I'm not completely comfortable with
6 this, and I don't know whether my colleagues are
7 completely comfortable with this; but what we are hoping
8 to do -- well, okay.

9 Before I even say this, let me say that
10 when we first got together and we were all in town here,
11 this was one of the things that we began talking about
12 was the validity of subspecies, what's a good subspecies
13 definition. And one of the first things that Scott
14 pointed out, which I think was a really good point, is
15 that none of us in our work today spend any time naming
16 new subspecies. It's not something that we do. It's
17 not -- it's not that we don't think that these things
18 are important, but we tend to focus on some newer
19 definitions, things like evolutionary significant units,
20 management units, a number of other things that have
21 been defined in the literature and may be more
22 operational or less operational.

23 And some of these -- some people will
24 argue equate with subspecies, some of these things some
25 people will argue they equate with species; and again,

1 we're left at a little bit of a dilemma because we don't
2 know exactly what a subspecies is. But what most of us
3 spend our time working on today is not naming new
4 subspecies but trying to figure out where these other
5 categories or distinct populations fall on this higher
6 -- I shouldn't even say hierarchy, maybe a continuum
7 that we find out there in nature.

8 And it's because most of us are
9 evolutionary biologists and we're interested in the
10 evolutionary history of these organisms and what that
11 might mean rather than the names, per se. And the names
12 are very, very important to the work that we do, and we
13 all have to agree on names, and we have formal
14 structures for applying names and rescinding names; but
15 most of us are most interested in the evolution of these
16 groups.

17 Before I go on, let me just see if my
18 colleagues have anything to add or correct.

19 DR. STEPPAN: I had -- it occurred to me
20 that as I was making my last comment, I saw a few
21 scrunched up faces like that was a rather distasteful
22 thing to deal with in terms of the squishiness of it,
23 but realize that even within evolutionary biology, there
24 are a lot of evolutionary biologists who really hate
25 dealing with the issue of species concepts and let alone

1 is taxonomy, and that is how do we classify diversity
2 and provide names and in some way describe that
3 diversity. Historically it was just classification.
4 More recently, it's actually phylogenetic history and
5 looking at, from a cladistic standpoint, clades and
6 lineages.

7 Well, these two or even three sets of
8 perspectives meet at the level of species, and species
9 are the fundamental unit of taxonomy, but the
10 definitions of the utility of species to taxonomists and
11 to phylogeneticists are not necessarily the same
12 characteristics that make them meaningful to
13 microevolution population geneticists.

14 And so there is this, I think,
15 fundamental dichotomy in the duality at least of species
16 as a concept. They have two to three very distinct
17 roles in how they -- what they play in evolution of
18 biology, and that doesn't even deal with when you get
19 into management issues. And so that's why the community
20 itself has actually had an expansion of species concepts
21 over the last 20 years as new data sets have been
22 developed. All the molecular data now allows us to
23 explore species as units in much more detail and a very
24 different approach, and it's called -- you know,
25 raised -- identified physical difficulties in the old

1 concepts. And so the result has been, in some ways,
2 even more confusion because we're now recognizing many
3 aspects of species.

4 So I think one reason why we're not going
5 to give a definitive answer here -- because we're not
6 all just nuts and we're not all just crazy -- is that
7 there's this duality of species; and in some ways,
8 people will never resolve this duality and bring it to a
9 single unit. Some people have favored a single
10 philosophical framework for all evolutionary biology,
11 but the cladistic framework within which you can unify
12 it all into one concept or one utilitarian purpose, but
13 not everyone agrees that that is the only way in which
14 we can view all of life from population to life as a
15 whole.

16 So that also leads to the difficulties
17 with subspecies, and this is why -- I just want to sort
18 of do that as a background. You know, again we're not
19 just nuts. There's this fundamental problem that we
20 probably will never be able to solve and people will
21 usually sort of find those definitions that are most
22 useful to the questions at hand or to which they have a
23 particular philosophical belief as being the most
24 important and then build their research program around
25 that.

1 DR. COURTNEY: I would add that, you
2 know, the law sold all this for us and defined species
3 for us in 1973 and then, you know, it looks very
4 different from either of the concepts. So I'm being
5 facetious, but essentially we've got that added layer of
6 three different languages, you know, being used
7 to -- using the same term for they're actually mutually
8 not translatable.

9 DR. DUMBACHER: Just speaking for myself
10 right now, I think that with the current usage of
11 taxonomy in birds, and from what I understand in mammals
12 too, many biologists would agree that there are
13 significant units below the level of species that
14 probably, you know, are very important evolutionarily
15 and should be recognized. But I think that, you know,
16 the difficulty here -- and I think that a large part of
17 the disagreement in these different studies was that
18 exactly, you know, what we consider significant. That's
19 a matter of opinion; and you know, we have to figure
20 out, you know, how -- how to draw that line.

21 And I think what -- what the panel is
22 beginning to think we're going to do is rather than
23 actually tell you where we think you should draw the
24 line, I think we're going to try to pull as many of
25 these definitions from the literature of subspecies, of

1 species, of ESUs, management units, you know, as many of
2 these things as we can find. And then ask, well, do the
3 data meet the criteria for this particular definition
4 and do the data meet the criteria for this particular
5 definition.

6 And I'm not sure -- and I'm pretty
7 doubtful, at this point, that we will weigh in and tell
8 the larger community where we think the line should be
9 drawn, but I think that we will do our very best to arm
10 the Fish and Wildlife Service with our interpretation of
11 how it meets these various definitions. So that's what
12 we're thinking at this point of doing. And I'll ask my
13 colleagues here to see if there's any --

14 DR. COURTNEY: There was -- well, a
15 raised comment.

16 DR. RAMEY: That's fine.

17 DR. STEPPAN: One of the other things, by
18 the way, that occurs to me in my little background
19 material there is that one of the things that
20 there's -- there are formal -- there are formal --
21 there's a formal role for species in taxonomy and that
22 is actually governed by the name and how we deal with
23 names and types and all that. The validity within the
24 nomenclature usage, however, is very well spelled out by
25 international conventions. So at the level of the

1 biological meaning of things, we argue quite a bit at
2 the level of how to deal with nomenclature that's
3 less -- that's not nearly as debated.

4 DR. DUMBACHER: And so one of the things
5 that might be important for us to gather from folks here
6 in the audience is if you know of particular definitions
7 out there that you'd like to make sure that we
8 consider -- you know, I mentioned a few of these
9 already, and we will be looking at Moritz' definition of
10 evolutionary significant unit, which has been brought up
11 already. Many people think there's already the level of
12 species; but because it's one of the levels that we can
13 address, I think that we will be addressing it.

14 But if there are any other comments or
15 thoughts out there, we would be more than happy to take
16 these, and we'll do our best to consider them as we
17 write up our final report.

18 DR. STEPPAN: I have a few more things.
19 Yes?

20 DR. CRANDALL: Do you have Moritz 2002
21 from Systematic Biology?

22 DR. STEPPAN: I do not have 2002.

23 DR. CRANDALL: Because it's a more
24 updated and synthetic ESU discussion.

25 DR. DUMBACHER: I think I have that one,

1 and we have your work as well.

2 DR. STEPPAN: Does that -- does he
3 actually there discuss ESU related to subspecies?

4 DR. CRANDALL: Yes.

5 DR. STEPPAN: Because I was looking at
6 '94 here and --

7 DR. CRANDALL: Do you want me to make --

8 DR. STEPPAN: -- he does not make any
9 equivalents between the two.

10 DR. CRANDALL: No. He explicitly does
11 not equate them, and he basically says identification.

12 DR. COURTNEY: Before you go too far, the
13 folks in the back are not going to be able to hear you,
14 and I wonder if you could maybe just come up and speak
15 into the microphone.

16 DR. CRANDALL: So this is from Moritz
17 Systematic Biology, volume 51, page 240. He says,
18 "Identification of intraspecific units for conservation
19 and management was originally based on taxonomically
20 recognized subspecies, a practice that continues to the
21 present. However, over the past two decades, several
22 deficiencies of this approach have been noted, including
23 uneven application of a subspecies category across taxa,
24 inadequate and confused criteria, and frequent
25 misalignment between historical subdivisions as revealed

1 by molecular assays in current boundaries of
2 subspecies," with the reference to Avise and Ball there.

3 And O'Brien and Mayr, "The concept of the
4 evolutionarily significant unit, ESU, was introduced to
5 guide prioritization of intraspecific units for captive
6 management, Ryder, 1986." And then adopted as a more
7 general term for distinct -- quote, distinct population
8 segments, and in parenthesis, of subspecies, which
9 qualify for protection under the U.S. Endangered Species
10 Act, Waples '91 and '95. For the latter purpose, an ESU
11 was proposed to have, as basic properties, some level of
12 reproductive isolation and adaptive disease."

13 So then he goes on and talks about how
14 the debate continues with ESU, and then he summarizes a
15 variety of ESU concepts; but it's clear that he
16 considers ESU an intraspecific designation, not
17 equivalent to species level.

18 And I would be the first -- and, in fact,
19 you know, our paper in 2000 was critical of this concept
20 because it's a hugely stringent concept to require
21 reciprocal monophyly.

22 DR. DUMBACHER: I know a number of
23 authors that have done that as well.

24 DR. STEPPAN: But categorized it as --
25 Moritz' thinking that that's the same as a species

1 concept is a complete mischaracterization of his work.

2 DR. DUMBACHER: Or as a subspecies, did
3 you mean?

4 DR. STEPPAN: He's clearly -- monophyly
5 of species, he's clearly thrown out subspecies
6 altogether and said we want -- you know, we should be
7 looking at ESUs, and then he does list his criteria. He
8 says: "In the following, I discuss these in turn using
9 reference points in the criteria I put forth earlier,
10 Moritz 1994a and Moritz, et al., 1995 and 1999,
11 recognizing that these -- recognizing that these have
12 been widely" -- and then in quotes -- in parentheses,
13 "(often with considerable latitude, with the reference
14 to our paper, and have proved to be something of a
15 lightning rod for debate.

16 "In brief, the Moritz 1994a, I propose
17 the following, and the first one is: The goal is to
18 assure the major historical lineages with recognized
19 species are protected and that the evolutionary
20 potential inherent across set of these ESUs is
21 maintained. Emphasis was placed on delineating sets of
22 populations that are historically isolated from others
23 rather than on current adaptive diversity. A second
24 category, management units, was suggested to recognize
25 demographically distinct populations that should be

1 managed to ensure the viability of the larger ESU.

2 The criteria for recognition of these
3 conservation units were that: One, ESU should be
4 reciprocally monophyletic for mitochondrial DNA alleles
5 and show significant divergence of allele frequency at
6 nuclear loci. And two, management units, MUs, should
7 have a significant divergence of allele frequencies at
8 nuclear or mitochondrial loci regardless of the final
9 genetic distinctiveness of the alleles." Thanks.

10 So you have that paper?

11 DR. DUMBACHER: Yes, I do.

12 DR. ARBOGAST: So just again to
13 reiterate, our goal is to have a broad survey of these
14 different definitions and maybe the same author and
15 different papers having slightly different definitions
16 of these, and we'll have them dated chronologically and
17 we'll just see if they meet them or not.

18 DR. STEPPAN: I think it's interesting
19 that he characterized his '94 paper as requiring
20 reciprocal monophyly because he also says in the 2004
21 paper -- and maybe he makes that final distinction
22 elsewhere -- "However, this criterion of reciprocal
23 monophyly may be too stringent given the
24 well-characterized species of its paraphyletic.
25 Mitochondrial DNAs have been well documented by Cronin

1 '93. A less stringent criterion would be significant,
2 but not necessarily absolute biogenetic separation of
3 alleles between populations. This could be assessed by
4 comparing the distribution of alleles among population
5 units compared to geographically randomized trees.

6 DR. KING: If you refer in that same
7 column --

8 DR. ARBOGAST: I think maybe it would be
9 best if you would come up and speak into the mic, if we
10 can do that.

11 DR. KING: All I want to do is just read
12 further from where Scott left off. This is -- this is
13 Craig Moritz' other paper in 1994 where he seems to be
14 backing away from the reciprocal monophyly for ESUs, and
15 he states -- I'll just read the whole paragraph -- "As
16 stressed above, it is important to seek corroborating
17 evidence from nuclear loci. Avise and Ball suggests
18 that there should be congruent phylogenetic structure
19 for other genes. However, alleles of nuclear genes are
20 expected to take substantially longer to show
21 phylogenetic sorting between populations or species
22 because of their typically larger affected population,
23 size, and slower neutral mutation rate. Nonetheless, it
24 is reasonable to require that ESUs defined by
25 significant phylogenetic structuring of mtDNA, also

1 shows significant differences in allele frequencies at
2 nuclear loci.

3 DR. DUMBACHER: So I hope that everyone is
4 aware of how difficult our job is going to be on this
5 because not only are there multiple definitions, but the
6 definitions change and even the authors of the
7 definitions disagree over the period of a few different
8 papers. Now, some of these can be fairly subtle, and
9 we're going to do our very best to include as many of
10 these different definitions and as many of the key
11 points. Because maybe what may be more important than
12 the definition, per se, is that there's several criteria
13 laid out in these various definitions, and we'll see how
14 each one of the data set fits each of the criteria, and
15 then maybe we'll then visit all the definitions and see
16 how the criteria fit the definitions and try to come up
17 with something.

18 DR. STEPPAN: It was pointed out that
19 Moritz had more than one '94 publication. The one to
20 which he may have been referring in 2002 may have been
21 the other one.

22 DR. CRANDALL: He refers to both of them,
23 so -- and he explicitly addresses the idea that the
24 reciprocal monophyly is too stringent of criterion; so
25 he says, "The reciprocal monophyly criterion has also

1 been deemed too stringent because distinct species that
2 have arisen from recent and rapid adaptive divergence do
3 not qualify as ESUs," and cites a variety of papers.
4 "However, these criticisms overlook the fact that ESUs
5 are intended to compliment recognized species, not
6 replace them."

7 So he's saying if you have a recognized
8 species that happens to not be monophyletic,
9 reciprocally monophyletic with its sister species,
10 that's fine, they're still good species. But you know,
11 we're not talking about species here. We're talking
12 about subspecies.

13 "Thus, recognized but recently divert
14 species of cave spider" -- and cites our paper because
15 that's the example we used -- and Behrs and another
16 paper -- "should be protected even though they appear as
17 monophyletic lineages within a larger paraphyletic
18 sister taxa." Now, that's all relative to well-defined
19 species.

20 DR. STEPPAN: So I think, Roy, you had a
21 comment. And I just have one final clarification I was
22 going to add to that, which is that again Moritz -- if
23 we're using Moritz as a source -- is talking about ESUs,
24 and ESUs are within species but not necessarily
25 equivalent to subspecies. It's an alternative way of

1 looking at -- within species diversity.

2 DR. RAMEY: I wanted to bring to the
3 attention a recent published paper by Matt Cronin to --
4 his intention was to eliminate redundant terminology in
5 intraspecific groups. I haven't had a chance to
6 thoroughly go through that, there's a hardcopy. You can
7 download it though.

8 I've been grappling with these issues for
9 a while as well, and one thing that I've realized is
10 that some of the subspecies concepts that were around in
11 the '40s, for example, the 75 percent rule and others
12 came along before the invention of high-resolution
13 molecular markers. And so, you know, a question for you
14 is that how do we incorporate sort of this basic
15 subspecies concept that was laid out for the technology
16 in computing power that evolved to this particular
17 point.

18 So it's that question of, what is
19 statistically significant versus what is biologically
20 significant. And they didn't have the tools at the time
21 in 1949, for example, to obtain that kind of resolution
22 that we can today at very low levels.

23 DR. DUMBACHER: So if I can address that a
24 little bit. The Patton and Union paper that was
25 published in 2002, and it was precisely to -- in lieu of

1 new molecular data, how could the 75 percent rule be
2 applied more carefully or uniformly; so it is something
3 that's more recent.

4 And there's a new paper by Waples, I
5 think you're aware of, from Molecular Ecology; and he
6 talks precisely about this issue that we currently have
7 so much power to distinguish between different --
8 different units in nature. The real question is, is our
9 power -- is our -- are our statistics more powerful --
10 or I should say, are there things out there that are
11 statistically significant that aren't biologically
12 significant; or is it vice versa, are there things out
13 there that are biologically significant that we're not
14 able to detect statistically.

15 And he talks quite a bit about that in
16 that paper, and this is something that we will be
17 looking at and trying our best to consider.

18 DR. RAMEY: And one thing, I've read that
19 literature too, particularly the applications on the
20 tests for use of structure is that the general consensus
21 is that using ten microsatellite markers, one can
22 generally get a high level of assignment probability to
23 populations; but in looking at that literature,
24 including the Waples paper, I haven't seen any reference
25 of utilizing those methodologies beyond the level of

1 populations to subspecies. It doesn't appear in there
2 in any of the papers I have on that. Thank you.

3 DR. DUMBACHER: Yes, thank you very much.

4 DR. KING: I think that we should state,
5 for the record, that I think Craig Moritz would be the
6 first one to tell you that he's not a molecular
7 systematist or a taxonomist, so I just wanted to state
8 that for the record. That's part of the reason why we
9 have -- have this dichotomy is that there are two
10 groups -- two groups of geneticists out there with
11 totally different philosophies. And folks like Craig
12 and Dr. Crandall and others have tried to bridge that
13 riff between the geneticists. It's a very difficult
14 issue.

15 DR. ARBOGAST: So I think one of the
16 things that is going to be addressed, hopefully in the
17 phone call with Jim Patton, will be some of the actual
18 nomenclature procedures that are involved. And it sort
19 of emphasizes the fact that there is one sort of world
20 in where taxa are named officially and legally, and
21 there are rules governing how they are treated and named
22 and renamed and so on, and that that can often be fairly
23 separate from these units that we would construct based
24 on molecular genetic data. And so we hope that he'll be
25 able to give us at least his perspective on that and

1 maybe help inform this debate.

2 DR. COURTNEY: Do you have any questions
3 of anybody in the audience? I'm thinking since we've
4 got Dr. Crandall here, he's been a party to a lot of
5 this debate, published a number of things. We have 20
6 minutes before we bring Jim Patton on unless we ask him
7 to come on earlier. Are we at a point where we should
8 do that or are you -- do we want to bring Dr. Crandall
9 up or are there things -- what does the panel want to
10 do?

11 DR. ARBOGAST: It would seem to me it
12 might be the best to talk to Jim Patton and let people
13 have responses to that if they -- unless they have
14 something they'd like to bring up before then.

15 DR. COURTNEY: Okay. So let me see if I
16 can get Dr. Patton on the phone a little bit earlier.
17 So why don't you take a five-minute break and stretch,
18 and I'll get him on the phone.

19 (Recess taken from 11:11 a.m. to 11:24
20 a.m.)

21 (Dr. Patton appeared telephonically.)

22 DR. STEPPAN: Hello. Hi, Jim, welcome.
23 Thanks for calling. Thanks for getting through. This
24 is Scott Steppan.

25 DR. PATTON: Hi, Scott, how are you?

1 DR. STEPPAN: Okay.

2 DR. DUMBACHER: Hi, Jim, this is Jack
3 Dumbacher. How are you doing?

4 DR. PATTON: Good.

5 DR. ARBOGAST: And, Jim, this is --

6 DR. PATTON: Doing all right.

7 DR. ARBOGAST: -- this is Brian Arbogast.
8 Thank you for calling in today.

9 DR. PATTON: Sure, Brian.

10 DR. STEPPAN: So, Jim, we've got you on
11 speakerphone. We've got Mike hovering over the phone,
12 and we're having a conversation here.

13 DR. PATTON: Okay.

14 DR. STEPPAN: So I know you raised -- had a
15 couple of issues you wanted to present that are
16 different perspectives on a lot of what we've been
17 talking about, so I wonder if you can just summarize
18 your main argument.

19 DR. PATTON: Well, so I don't have an
20 advocacy position one way or another as to this
21 particular case. The issues that I raise have more to
22 do with -- well, how I personally would have gone about
23 trying to evaluate the status of this taxon or any other
24 taxon, but also to kind of the legalistic aspects of how
25 to evaluate a -- what is a formal name in zoological

1 nomenclature that's already on the books, not trying to
2 ascertain whether or not there are subspecies out there
3 in the first place.

4 And so the issues that I raised in the
5 little note that I sent to you guys yesterday had to do
6 with that aspect. If a taxon is already on the books,
7 it's a formally recognized name. If it was defined,
8 described some time in the past, then minimally the
9 characters that were used to diagnose that taxon and
10 hopefully the set of specimens that, in fact, were at
11 hand by the describer, need to be evaluated. And if
12 that's not done and yet, you know, some other definition
13 or some other set of characters is applied, it's kind of
14 like, you know, comparing apples and oranges.

15 And so the question I asked myself when I
16 reviewed the various treatises that have been done on
17 this taxon is whether or not the original definition and
18 diagnosis of *preblei* has been adequately tested, and I
19 don't think that it was and I think that it should be
20 done. That would be the first step that I, as a
21 systematist, would have done. And then, depending upon
22 the outcome of that analysis, one could broaden both the
23 methods of analysis as well as the kinds of data that
24 might be applied to either further -- to try and
25 interpret the distinctiveness of this entity or the lack

1 of distinctiveness of that entity.

2 And so that's the kind of methodological
3 procedure that I would have gone through, and that's the
4 point that I wanted to convey; that there's a systematic
5 procedure that would have been, I think, followed by
6 most systematists who work at the infraspecific level
7 with regard to asking this question in the first place.
8 Is that what you wanted?

9 DR. STEPPAN: Yeah. And so to what
10 extent is that sort of -- if you just sort of clarify
11 the way you would go about doing things as a preference
12 and to what extent is that sort of required by code of
13 nomenclature.

14 DR. PATTON: It's not required by the
15 code of nomenclature at all. There is nothing in the
16 code that specifies how one goes about to evaluate a
17 specific taxon. But my point is is that if we're
18 starting with a clean slate, okay, and we wanted to
19 define, you know, variation in nature, then it's
20 possible to, you know, come up with enough priority set
21 of rules by which we might be able to limit geographic
22 units.

23 Now, everybody's going to differ as to
24 what -- they'll disagree as to what those rules might be
25 or where to draw boundaries; but if you're starting with

1 a clean slate, then you can, you know, come up with a
2 scheme. But we're not starting with a clean slate for a
3 previously described taxon that is formally recognized
4 in the literature, all right. So that taxon is already
5 there, and it has to be evaluated on its own basis.

6 It would be kind of like saying that, you
7 know, I've got a car and it's manufactured by Ford, but
8 I've got another car over here that's a sedan. Because
9 it's a sedan, it's not a Ford. And that's -- you can't
10 make that comparison because the original definition of
11 the car that you had in the first place was its
12 manufacturer, not its style. Do you understand what I'm
13 saying?

14 And so that's why one would have to go
15 back with an existing taxon, one would have to go back
16 and at least minimally start by evaluating the criteria
17 upon which that taxon was based in the first place.
18 That's just the kind of standard systematic procedure,
19 has nothing whatsoever to do with the code of
20 nomenclature.

21 DR. STEPPAN: So how would you go about
22 testing the validity? And so this is --

23 DR. PATTON: Well, I wouldn't -- it's
24 not a matter -- so much a matter of testing validity.
25 It's a matter of evaluating the statements, the truth in

1 the statements of the diagnosable characters that Phil
2 Krutzch used in his original description.

3 So I looked at his monograph yesterday,
4 and I may be off one or two, but there's something,
5 like, you know, 8 or 9 -- I mean, 9 or 10 or 11
6 characters, depending upon how one reads his monograph,
7 that he used to diagnose preblei relative to campestris.
8 And in his diagnosis, he explicitly states that he is
9 comparing his haplotype, his type series, the 11 or so
10 specimens that he had, to topo types of campestris, and
11 he lists those characters. And four or five of them are
12 caudal color characters; and four or five of them are
13 cranial characters, only a couple of which are
14 mensural, the rest of them are qualitative, you know,
15 fully large or less inflated frontal -- frontal region,
16 less inflated or more inflated. I forgot which, okay.

17 And so that set of characters, therefore,
18 defines that taxon in relation to campestris; and that
19 set of characters, therefore, must be evaluated at face
20 value with additional samples.

21 And so the issue to me, if I was sitting
22 in your shoes, the issue to me would be has that
23 evaluation been done, yes or no. And if it has been
24 done, what are, you know, the conclusions of that
25 evaluation. Does additional samples refute or falsify

1 the original differences that Krutzch saw between those
2 two taxa or does -- does that evaluation or reanalysis,
3 you know, support that.

4 And I suspect that if one were to do that
5 analysis, you know, you'd find that some characters
6 probably continue to diagnose preblei really and
7 campestris and some may not. And then you have to come
8 to some conclusion about, well, how many do you need and
9 how would you expand an analysis to include different
10 character sets and so forth to further address the
11 distinctiveness of preblei.

12 DR. STEPPAN: On a related aspect, how
13 many of the characters need to be thoroughly evaluated?
14 So -- so, for example --

15 DR. PATTON: It's in the eyes of the
16 beholder, my friend, as you well know as the rule.

17 DR. STEPPAN: Right.

18 DR. PATTON: And there can never be in any
19 kind of rule, and I mean, it's ridiculous to -- for
20 people to think that there should be some kind of rule.
21 You know, I mean, a case at point that I also raised
22 yesterday in that little memo are these melanistic
23 subspecies of the rock pocket mouse. That's basically a
24 single character. Is that adequate to diagnose, you
25 know, a distinct taxon. One could argue one way or the

1 other, and without coming to my own -- giving my own
2 personal opinion, it has been argued one way, you know,
3 in both directions and the current taxonomy lists those
4 subspecies as well. Well, somebody out there thinks
5 that the -- you know, basically a single character's
6 adequate.

7 DR. STEPPAN: So following up on that,
8 how -- in this case I'll sort of ask your opinion, so
9 I'll throw out a few hypotheticals here to sort of
10 explore the range of possibilities. So if you have an
11 original description, for example, that's rather
12 ambiguous in its definition. You know, how thoroughly
13 should an ambiguous character be evaluated?

14 So I'll just throw out a totally extreme
15 case where the only defining character is the coloration
16 pattern, and if the coloration pattern is actually --
17 and this is to the extreme -- that *preblei* has the color
18 of an evening sunset whereas *campestris* is more like a
19 sunrise orange on the sides. Now, that may have meant
20 something to Krutzsch at the time, but you know, people
21 coming along later may look at that and have no idea
22 then how to evaluate that.

23 DR. PATTON: That's true.

24 DR. STEPPAN: To what extent does that
25 have?

1 DR. PATTON: And I don't have any idea of
2 what Krutzch meant by evening sunset or whatever the
3 other, you know, term you've used; but it is possible to
4 go to the specimen that Krutzch looked at and use a
5 colorimeter and actually measure, you know, the color
6 variables from those same specimens so that you can get
7 a quantitative measure of color that Krutzch was trying
8 to -- that Krutzch visualized and was trying to
9 articulate verbally. Hello?

10 DR. STEPPAN: Yes. I was thinking of the
11 next question. So would you say it's true that in many
12 cases, historical revisions have done -- people who have
13 done revisions have oftentimes synonymized subspecies
14 after a relatively cursory evaluation of the prior
15 taxonomy?

16 DR. PATTON: Yeah, absolutely. But I
17 would also say this, that, you know, if you just kind of
18 look at the history of taxonomy in revisionary studies
19 in small mammals, you know, many of these taxa were
20 initially described as full species, then they were
21 synonymized and recognized as a subspecific basis or
22 level, if at all, and most of that work was done in the
23 '30s, '40s, and '50s, maybe early '60s. And there's
24 been very little in the way of revisionary work, you
25 know, applying kind of modern -- a modern methodology

1 and a modern paradigm or a new paradigm to most of these
2 kind of intraspecific revision -- I mean, at the
3 intraspecific level to most taxa of small mammals even
4 here in North America.

5 So that most of the taxonomy that we
6 have, if you look at, you know, the most recent mammal
7 species in the world, most of that stems from
8 revisionary work that was done in the '40s and '50s,
9 maybe '60s under a specific paradigm that is largely,
10 you know, no longer the paradigm that we would use today
11 if we were to start from scratch.

12 DR. STEPPAN: So for -- essentially how
13 does the community look upon sort of revisionary studies
14 that maybe have not done what some people might consider
15 a thorough evaluation. Oftentimes those -- would you
16 characterize some of them are -- become evaluated as
17 sort of the authoritative reference being the more
18 recent revisions despite what may be sort of a lumping
19 approach?

20 DR. PATTON: I think that you've
21 got -- there's several things that are involved here. I
22 mean, there's a section of the community that, you know,
23 basically ignores kind of revisionary studies except at
24 the species level, either because they're
25 uninterested -- I mean, their own, you know, research

1 directions are not focused on geographic variation and
2 they're uninterested in that; or they just don't believe
3 that, you know, basically a continuum can be divided up
4 in some fashion.

5 So I can think of, you know, Jim Findley
6 when he did mammals of New Mexico 20 years ago or 30
7 years ago -- and Findley, of course, was one of Hall's
8 students. And, you know, his own thesis work was on,
9 you know, shrews of the *Sorex vagrans* complex where he
10 dealt with species and subspecies and so forth. In his
11 mammals of New Mexico, he refused to, you know, to
12 document the subspecific units because he just didn't
13 think that it was possible to do that. So you've got
14 that side of the spectrum.

15 Then you've got, you know, folks like me
16 who have said -- you know, who follow more of a
17 Granellian kind of approach who said there are lots of
18 variation out there in nature and is there a way to
19 compartmentalize it. I mean, are there truly, you know,
20 geographic units that can be recognized even if the
21 boundaries are fuzzy and is there value in recognizing
22 those units; and my personal opinion is yes, there is.
23 So you do the best you can with the data that you have
24 available to you to define what those units are.

25 But I would say that most of the

1 systematic community now is so focused on simply using
2 molecular tools, that they have lost -- you know, that
3 they've lost interest in the formal description of
4 subspecies, and in many cases even of species because
5 they're working on the higher levels. And so there's a
6 real mixture in the community at large.

7 DR. STEPPAN: So a different vein, so as
8 someone who's worked fairly extensively both in
9 molecular and morphological approaches, to what extent
10 do you see congruence at the intraspecific level? So do
11 you have species -- subspecies that you consider to be
12 valid descriptions of geographic variation based on,
13 let's say, morphological data, to what extent is it
14 congruent with molecular data, and vice versa?

15 DR. PATTON: Well, again, I mean, to a
16 large extent, there's reasonably -- I mean, if I go back
17 to -- over revisionary studies that we've done, you
18 know, we ended up defining, to a large extent, the
19 subspecies that we recognized because of the congruence
20 between morphology and molecules. And of course in that
21 day, you know, the molecules are allozymes rather than
22 DNA sequences. If we applied DNA sequences to those
23 same units, we'd -- particularly mitochondrial DNA --
24 we'd get much more local diversification; but the
25 boundaries that we recognized at the subspecific level

1 would still be, you know, units that would be recognized
2 at the sequence level.

3 But I can think of many cases in which,
4 because divergence is relatively recent, that the
5 typical markers that we are using now, like
6 mitochondrial sequences or even microsatellites, they
7 haven't sorted, you know, reciprocally monophyletic
8 units; whereas the morphology, clearly documents, you
9 know, regional uniformity and sharp discordances over
10 narrow geographic areas that one could use, you know, to
11 mark subspecies boundaries.

12 So you've got a jump rope continuum and
13 you've got a geographic continuum and sometimes the data
14 sets all line up and sometimes they don't. And the
15 decisions of the taxonomists or the systematists is --
16 in fact, the dilemma to this system, is, in fact, you
17 know, adjudicating where those differences lie and at
18 what level one wants to recognize the boundaries.

19 I can give you lots of specifics of cases
20 where at the mitochondrial or even microsat level
21 there's little differentiation but at the morphological
22 level substantial differentiation. I can also give you
23 cases at which you get the wrong signal of history from
24 the mitochondria simply because of introgression events
25 or selective sweeps where you've got extreme, strong

1 morphological discordance and introgression of
2 mitochondrial, genome that wouldn't belie that
3 morphological discordance.

4 DR. STEPPAN: So I have one very general
5 question I was going to still ask on my list. Does
6 anyone else on the panel right now have any specific
7 questions?

8 So one of the questions we had wanted to get your
9 perspective on, because we just had a little discussion
10 here before you called, on subspecies concepts; and we
11 were curious what your perspective was or how you would
12 characterize probably the common or most widely applied
13 approach to subspecies in the mammalian community today.

14 DR. PATTON: Well, I mean, that's a
15 difficult question to address because it depends on,
16 again, if we were to use a clean slate today and start
17 over again. I think that most of the systematists would
18 analyze or, you know, employ as wide a diversity of
19 characters, both morphological and molecular as
20 possible. And define boundaries, recognizing that those
21 boundaries are going to be fuzzy, but define boundaries
22 where there are sharp discordances over short geographic
23 areas. So steps in morphological climbs or even steps
24 in molecular climbs relative to, you know, networks and
25 so forth.

1 I don't think that, you know, most
2 systematists would expect any kind of reciprocal
3 monophyly for mitochondrial genes, for example. I mean,
4 that's something that folks would look for at the
5 species level maybe, but not at the subspecies level.
6 They might look for, you know, uniqueness of haplotypes
7 in one particular taxon or one particular geographic
8 unit, but not reciprocal monophyly, per se.

9 But if we go back in time and recognize
10 that the specific nomenclature that's facing us right
11 today was built on a different paradigm, we can't ignore
12 that paradigm. We can't ignore that history, and
13 that's, I guess, the major point of what I was trying to
14 write yesterday. And so you have to kind of do a dual
15 level of evaluation. You have to evaluate the original
16 definition before you can apply a new definition rather
17 than just simply apply a new definition to -- to an old
18 one.

19 DR. STEPPAN: So another thought too. In
20 your perspective, to what extent does the length of time
21 that a set of populations have had an independent
22 evolutionary history or had some sort of separation play
23 into your view of subspecies?

24 DR. PATTON: It could be almost
25 instantaneous. There doesn't have to be any substantial

1 length of time because it depends on the evolution of
2 the characters that you're using to define these things
3 in the first place, okay. It's conceivable that you can
4 have a character that is under extreme, strong
5 directional selection that can generate uniformity over
6 a geographic area in that character because of that
7 selection in a very short period of time. We all know
8 that.

9 And, you know, whether or not we want to
10 recommend -- again, getting back to the question as to
11 whether or not we want single characters or few
12 characters to define intraspecific taxa, that's a
13 personal argument. But it is conceivable to do that,
14 and it's conceivable for such to occur over such short
15 periods of time.

16 DR. STEPPAN: Okay. Does the panel have
17 any other questions?

18 DR. DUMBACHER: Hi, Jim, this is Jack.

19 DR. PATTON: Hi, Jack.

20 DR. DUMBACHER: I have a quick question
21 for you. Just in talking about testing the original
22 hypothesis that was laid out in the original
23 description, I've seen this happen on a number of
24 occasions and -- where the original description, now
25 that we have many more specimens than were used in the

1 original description, we find out that some of those
2 characters, or maybe even all those characters, may not
3 hold but there may still be a genetic description there.

4 DR. PATTON: Absolutely.

5 DR. DUMBACHER: And just in how
6 taxonomists do their work will often put, you know,
7 hundreds or, you know, earlier it may only have been
8 dozens or even less specimens on the table. And we
9 might use a lot of other information about what -- why
10 these are different, and then writing up the
11 description, we'll try and find as many characters as we
12 can from the specimens in front of us that might be
13 useful to somebody reading our papers.

14 DR. PATTON: Absolutely.

15 DR. DUMBACHER: So when you were talking
16 about testing that original description and the accuracy
17 of the original description, if you find that the
18 original description doesn't hold but there's other data
19 suggesting that the author was correct for other
20 reasons, what do you think should be done in that case?

21 DR. PATTON: Well, I mean -- so I mean,
22 in the original -- every one of us, you know, has a
23 limited set of information in front of us, but that
24 doesn't mean that if new information comes along, we
25 can't use it and redefine, or as I would say, we would

1 have an amended diagnosis for the taxon. So the
2 original characters that were used to diagnose the taxon
3 may all be invalid, but there may be other attributes
4 that we're able to distinguish now that weren't
5 available to distinguish before, but we can use the
6 amended diagnosis. There's no conflict there.

7 What I was arguing is that you can't, to
8 me, at least -- you have to evaluate your original
9 definition of diagnosis. You can't ignore it and apply
10 a new definition of diagnosis in lieu of that. Is that
11 understandable?

12 DR. DUMBACHER: Yeah, absolutely. Thanks
13 a lot.

14 DR. PATTON: Yeah. The other thing that
15 I would say is that it's critical -- I mean, any -- any
16 systematists worth their salt, I think, would have
17 demanded that they look at the whole type and the type
18 series that was used in an original description if they
19 were going to make this kind of an evaluation. And so a
20 question to all of us, the various authors of various
21 papers in this particular dialogue, a question to them
22 is whether or not that was done. If that wasn't done,
23 then there is even more difficulty in understanding, you
24 know, the evaluations of Krutzsch's original hypothesis
25 because there are lots of well-known cases where there's

1 been morphological differentiation in short periods of
2 time, including decadal periods for small mammals.
3 There was one that was just recently published, you
4 know, for Alaskan *Sorex cinereus*, for example, over the
5 last 30 years where there's been substantial size change
6 and coordinate size change of the skull.

7 So it is important at the systematic
8 level to evaluate variations from the specimens that
9 were used in the original descriptions.

10 DR. DUMBACHER: Great, thanks.

11 DR. PATTON: Sure.

12 DR. STEPPAN: We normally -- if you had
13 been able to arrive, and certain of those that have
14 spoken here, there's been the opportunity for people
15 attending to ask any questions. So I'm going to briefly
16 ask, is there anyone who would have a question or raise
17 an issue for Jim Patton?

18 DR. DUMBACHER: Keith Crandall's going to
19 come up and ask a question.

20 DR. PATTON: Sure.

21 DR. CRANDALL: Hey, Jim, this is Keith
22 Crandall, how are you?

23 DR. PATTON: Hey, Keith.

24 DR. CRANDALL: In your proposal to
25 evaluate the original description and the original

1 taxon, you said you would add more samples to that,
2 right?

3 DR. PATTON: Oh, I'd use every -- I mean
4 today I would use every available specimen, sure.

5 DR. CRANDALL: So the question is: When
6 you have one subspecies that's in the mix of 12
7 subspecies within a species that's geographically
8 widespread, right --

9 DR. PATTON: Yeah.

10 DR. CRANDALL: -- across North America.

11 DR. PATTON: Yeah.

12 DR. CRANDALL: How do you design a
13 sampling strategy to evaluate that one subspecies, or
14 should you be evaluating that one subspecies?

15 DR. PATTON: Well, so I've never -- I
16 mean, that's a very good question; and it's not an issue
17 that I've ever dealt with myself because my work has
18 always been at the revisionary level, so I've looked at
19 the entire taxon. So in this case, you know, if I was
20 to have redone Krutzsch's work, I would have looked at
21 all specimens, at least as many as I could find, of
22 hudsonius throughout its range and reevaluated all of
23 the existing taxonomy as he lays out, preblei just being
24 one part of that. And so it's hard for me to, you know,
25 to specifically answer your question because I've never

1 done that. I understand what you're saying.

2 One of the great difficulties, of course,
3 is that, you know, we can't priori provide a sampling
4 strategy because the samples are those that are already
5 available to us based on museum collections. Now, we
6 can plot those localities and see where the gaps are in
7 the range and go out and try and/or see what the sample
8 sizes are for the existing localities and go out and try
9 and augment those sample sizes or find specimens from
10 interbreeding areas.

11 But I guess minimally if one were to go
12 back and think about the way Granell -- kind of my
13 hero -- went about trying to describe geographic
14 variation, you know, he looked at it as though it
15 was -- as though they were kind of plateaus or basis of
16 commonality separated by, you know, relatively sharp
17 steps in climbs over short geographic areas. And when
18 he saw that, you know, he would recognize those
19 plateaus, those different subspecies. Even though the
20 sharpness and the steepness of the climb or the
21 sharpness of the geography might vary, you know, from
22 one geographic area to the next.

23 So minimally, one would have to have the
24 samples that would be able to, you know, kind of address
25 that, how sharp the distinction is and over what, you

1 know, life of geography. But you're not always going to
2 be able to do that because you've got real gaps in the
3 geographic ranges. And I gather that's one of the
4 problems with preblei is that there's 100-some-odd
5 kilometer gap between it and campestris and even greater
6 gaps between it and the subspecies further to the east.

7 I'm not sure that that answers your
8 question, but I'm not sure that your question is really
9 addressable.

10 DR. CRANDALL: Yeah, that's great, Jim.

11 DR. STEPPAN: Are there any other
12 questions?

13 DR. RAMEY: Hi, Jim, it's Rob Ramey here.
14 How are you doing?

15 DR. PATTON: Hi, Rob.

16 DR. RAMEY: Thank you so much. Wish you
17 could be here with us.

18 DR. PATTON: Well, I'm not sure that I
19 want to be there.

20 DR. RAMEY: Join the fun. Here's my
21 question, if the original description was based on a
22 qualitative assessment without measurements, then
23 wouldn't a qualitative assessment of those characters be
24 adequate to question the basis of that original
25 assessment? And the point here is that many of these

1 taxa subspecies have been arbitrarily defined on traits
2 that were poorly defined on the -- in the first place
3 and that are not quantifiable.

4 DR. PATTON: Well, I'm not sure that I
5 would agree that, you know, that you should use the word
6 "arbitrary." And I certainly would not agree that
7 qualitative traits of priority are somehow less useful;
8 valuable; or even, you know, identifiable than a
9 quantitative trait.

10 There are several things. It is possible
11 to take traits that are qualitatively defined originally
12 and quantify them, okay. You can take shape parameters
13 such as, you know, bullae inflation, which is the
14 curvature of the bullae. It's, I think, one of the
15 characters -- or frontal inflation is one of the
16 characters that Krutzsch used. I mean, you could use
17 statistical analysis, 4A analyses, and so forth and
18 trace that inflation, that arc, okay. And so you can
19 apply modern techniques to actually get at, in a
20 quantitative way, that qualitative statement.

21 The same thing, as I said, is true for
22 the color differences that, you know, are standardly
23 noted. I mean, people in those days -- and still do --
24 they use Munsell, you know, color chips or they use the
25 Bridgeway color system, but all of those are

1 quantifiable. You could use a colorimeter to do that.

2 So the fact that they were qualitative in
3 the first place doesn't mean that you can't quantify
4 them; but even if you can't quantify them, you can
5 still -- any good systematist can lay out those
6 specimens and say yeah, on average, you know, these
7 things have larger X or smaller Y than this other set
8 does. And that, you know, could be repeated by
9 individual after individual after individual. So I
10 would not agree with a general statement that -- that
11 qualitative characters are somehow less valid than
12 quantitative characters are.

13 DR. RAMEY: No, I think you missed the
14 point there. I was really trying to iterate this -- if
15 qualitative basis was the original basis, then by the
16 same argument, a qualitative assessment, especially with
17 additional data from multiple lines of evidence, should
18 be adequate. But thanks for your opinion, Jim.

19 DR. PATTON: You bet.

20 DR. STEPPAN: Well, actually, can I sort
21 of take the first part of Rob's question and maybe
22 rephrase it? And this may not have been what Rob was
23 asking, but it's one that I'm curious about. So if the,
24 let's say, one or more of the characters are rather
25 imprecisely defined, is the level of the test perhaps a

1 bit lower, that is, if it's an imprecise definition,
2 does one have to do -- can one use certain qualitative
3 assessments to say that no, this does not actually seem
4 to be consistent or a characteristic?

5 DR. PATTON: Well, I guess my argument --
6 my simple argument would be I don't care how, you know,
7 precisely or imprecisely the character is stated in the
8 original description. If an author says X about a
9 particular character, then I think it's incumbent upon a
10 reviser to evaluate that character. Now, it may be
11 difficult to evaluate it as the -- the evaluator may
12 finally decide that it's not possible to evaluate it
13 because the level of precision in the description is so
14 poor, but that is a statement that has to come forth.
15 You can't opt to already make the decision to not look
16 at that character because you think that it has a
17 certain level of imprecision with regard to the way it's
18 stated.

19 DR. STEPPAN: And on a related point, to
20 what extent are some of these values? How do they
21 characterize population, as an example, averaging
22 smaller in most cranial measurements taken as one of the
23 comparisons to campestris? So what is it -- how do you
24 actually determine where preblei is smaller on average?
25 Does this simply require demonstrating that

1 there -- that the mean for the geographics of the
2 populations is statistically less; or is it that for
3 each population it's statistically smaller than each of
4 the populations in campestris; or just that the majority
5 of individuals in prebleii would be smaller, the majority
6 of individuals in campestris?

7 DR. PATTON: Well, remember, I just got
8 Krutzsch's monograph right here in my little hands, and
9 so I remember what he says. He is -- in that statement,
10 he says, from topo types of campestris, prebleii differs
11 as follows, okay. And one of those is averaging smaller
12 than most cranial measurements taken. That's comparing
13 his sample of prebleii to topo types of campestris and
14 not to the geographic variation within campestris.

15 DR. STEPPAN: So then how do you evaluate
16 that?

17 DR. PATTON: You have to go back -- I
18 mean, if you want to evaluate that statement, you have
19 to go back and do what Krutzsch did with larger -- you
20 know, if you've got larger samples of prebleii, compare
21 them to larger samples or to at least his samples of
22 topo types of campestris, all right. Now, that still
23 begs the issue they may be -- prebleii may be on average
24 smaller than topo types of campestris, but that still
25 begs the issue as to whether that difference in size is

1 final through that area or whether there's a step in
2 that climb, and that's where additional geographic
3 analysis, you know, would come to play.

4 DR. STEPPAN: Okay. Are there any other
5 questions? Jim, do you have any final thoughts you'd
6 like to leave us with or --

7 DR. PATTON: No, I just wish you luck. I
8 wouldn't want to be in your shoes. I don't think the
9 subspecies concept is one that science or the legal
10 system will ever -- you know, for which one will ever
11 develop a clear-cut and universally applied set of
12 rules. So good luck to you.

13 DR. STEPPAN: Okay. Well, thank you very
14 much for your time.

15 DR. DUMBACHER: Thanks, Jim.

16 DR. PATTON: You're more than welcome.
17 Yeah, enjoy yourselves.

18 DR. STEPPAN: We will, thanks.

19 DR. PATTON: Bye-bye.

20 DR. STEPPAN: Bye.

21 DR. COURTNEY: Okay. We've got a long
22 break. The panel says they want to take a -- both a
23 break now to talk amongst themselves and then lunch, and
24 so we're looking to see you back here at 1 o'clock.

25 (Noon recess taken from 11:06 p.m. to 1:04

1 p.m.)

2 DR. COURTNEY: All right. I think Lisa
3 just stepped out to do the whipping in to make sure that
4 everybody's here. So we've, essentially, reached the
5 point where we've heard pretty much what we need to hear
6 and at least we think what we need to hear. The panel
7 is going to read you a statement in a minute stating
8 where we are at the moment. And I guess before we do
9 that, I should say a couple of things.

10 Firstly, there's still one or two loose
11 ends that we're tying up. We're still going to look at
12 the chromatograms in Dr. Ramey's group, and we should
13 get those later this afternoon. I believe that we still
14 have a statement coming in from the Vignieri group on
15 morphological issues, and I was told it was on the
16 email; but I haven't seen it yet. And I think that's it
17 in terms of any last bit of information that we're
18 waiting for, right?

19 The -- essentially just so you understand
20 our process, we're under the gun. We don't have a lot
21 of time in order to fulfill our contract, and the Fish
22 and Wildlife Service has even less time once they get
23 our report to decide what to do with it; so we're pretty
24 much constrained into going where we are now.

25 If you have additional information or

1 additional things you want to bring forward to us, you
2 know, now is your opportunity. Actually, you know, my
3 son is getting married in about 10 days' time, so I feel
4 like I'm the guy standing up in the front saying speak
5 now or forever hold your peace. So quite seriously,
6 this is your opportunity to talk to us where -- we feel
7 pretty comfortable, at least the panel will tell you, is
8 comfortable where we are. If you have things to bring
9 to us, now is your chance.

10 So before we go any further, we should
11 give -- has anybody got stuff that they are wanting to
12 raise? Yes?

13 MS. ROBERTSON: I have the Bergstrom
14 printed out.

15 DR. COURTNEY: Yes, thank you.

16 DR. DUMBACHER: What are these again?

17 DR. COURTNEY: Thank you. Anybody else?

18 Any last statements, questions? In which case, then,
19 here's what we're going to do. You do?

20 DR. KING: Yes. I do want to say that
21 there is one paper out there that's been accepted for
22 publication in Conservation Biology that we haven't
23 discussed here and that is the paper by Sue Haig which
24 offers some guidance, suggestions on how to -- how to
25 deal with the subspecies issue with the underlying

1 Endangered Species Act. And if you haven't seen that,
2 I'll be happy to give you a copy.

3 DR. COURTNEY: Is that the one we looked
4 at as part of the spotted owl process? Because that was
5 a conceptual paper, it wasn't a spotted owl paper.

6 DR. KING: I don't believe so. This was
7 just accepted in the last month or so, a couple of
8 months ago, and it should be considered in the press.

9 DR. COURTNEY: Is that publicly available
10 or can we --

11 DR. DUMBACHER: I can write to Sue. I'm
12 corresponding to her about several other issues, so I
13 could ask her about it.

14 DR. KING: I have the PDF of it on my
15 laptop.

16 DR. COURTNEY: What I meant is can we
17 give it to everybody else?

18 DR. KING: I think so, yes.

19 DR. DUMBACHER: Yeah, that would be
20 great.

21 DR. KING: It should have some very -- I
22 didn't realize that we weren't going to be talking any
23 more this afternoon or I would have brought it up before
24 we broke up, but there's a copy of it.

25 DR. COURTNEY: Okay. Well, that looks

1 like it's relevant.

2 DR. DUMBACHER: Do you think you could
3 email a PDF when you get a chance to the Preble's?

4 DR. KING: Or if you have a flash drive,
5 I can put it on there right now.

6 DR. COURTNEY: I think just in the
7 interest of completeness, we should make sure before
8 everybody leaves that they have access to this stuff.

9 DR. KING: I'll be happy to do that.

10 DR. COURTNEY: But I think you can do it
11 directly or ask Lisa to photocopy.

12 DR. DUMBACHER: As we mentioned earlier,
13 we're going to try and examine a variety of different
14 approaches, and so if there's something new out there
15 that somebody sees as caliber, we'd be more than happy
16 to include that and do our best to include our
17 evaluation of it.

18 DR. COURTNEY: Okay. Anything else? If
19 not --

20 MS. ROBERTSON: Is the panel meeting with
21 Hsiu-Ping?

22 DR. COURTNEY: We're going to get ahold
23 of the doctor and have asked her questions by email. I
24 don't believe we've been on email to find out responses
25 yet.

1 DR. DUMBACHER: Yeah, I haven't been.

2 DR. COURTNEY: But that whole issue about
3 comments or questions about the Ramey group's data is
4 still part of our process. We haven't closed the door
5 on that.

6 DR. KING: I have a series of questions
7 to you for you to consider.

8 DR. COURTNEY: We'll certainly consider
9 this.

10 DR. DUMBACHER: For Hsiu-Ping?

11 DR. COURTNEY: Yeah. So remember
12 that, the two things that I said we were going to follow
13 up on. One being that issue and one being those things
14 that you've taken care of for us.

15 All right. One thing I have to tell you
16 is that Dr. Van Den Bussce, of course, wasn't able to
17 participate. I've left him messages. I don't think
18 it's a good sign that he didn't even to reply to those
19 messages, so obviously his personal family issues are
20 serious.

21 We talked over with the panel about what
22 best to do about that, and I guess the feeling was that
23 since he's not been part of this meeting and since these
24 panelists at least felt that things were moving in a
25 good direction, that we decided that we would ask him

1 questions but that he wouldn't get a vote from here on
2 out. You know, we might draw on him as a resource to
3 talk to about, you know, mammalogy issues but that,
4 essentially, the report would be written by the three
5 panelists you see in front of you.

6 Does anybody have any questions about
7 that? If not, then, you know, you've had your chance to
8 speak, and I'm going to let the panel read their
9 statement.

10 DR. DUMBACHER: Okay. On behalf of the
11 panelist, we just wanted to say we've had a marvelous
12 opportunity here to examine these very two -- these two
13 very large data sets, explore the differences in data
14 types, data quality, data quantity, different sampling
15 genes, as well as differences in the analyses and
16 interpretation.

17 U.S. Fish and Wildlife Services has also
18 provided a number of thorough opinions and critiques of
19 both bodies of work that have been extremely valuable to
20 us, and we considered them all.

21 Furthermore, we've had opportunities to
22 directly address all of our concerns and ask questions
23 of the principal investigators and other people who have
24 looked at these data. We think that both studies
25 contributed major advances in our understanding of the

1 biology and the evolutionary history of these organisms,
2 and we commend both Dr. King and Dr. Ramey for their
3 contributions to this effort. These large data sets,
4 sophisticated analyses represent a lot of hard work and
5 dedication to these issues.

6 And at this time, we think we have a
7 pretty good -- we have a good understanding of the
8 issues and the basis for the differences between these
9 two sets of conclusions and we'll explain this in detail
10 in our final report.

11 So we'd like to reiterate our
12 appreciation to all the people who have participated, in
13 particular to Dr. Ramey and Dr. King. We'd also like to
14 thank the audience for your questions and your
15 participation, and we appreciate everyone's effort to
16 keep this process as professional as it has been. So
17 thank you very much, everyone, and that's all we would
18 like to say at this time.

19 DR. COURTNEY: I, however, get the last
20 word, which is this is always a difficult thing in terms
21 of running these sorts of processes and this was a
22 charged issue and obviously it's been handled in an
23 extremely professional way, and I just want to commend
24 you all.

25 I also wanted to make it clear, the press

1 is still here. I think sometimes people look for
2 winners and losers, and I think we're the winner in this
3 case, and the winner is the scientific process, which
4 is, I think, honored by what we've all tried to do and
5 that what you've seen is a fair and professional
6 exposition of the facts, and there is no need to
7 identify a winner in personal terms. The winner is the
8 process that we've seen here.

9 And I also want to express my personal
10 thanks to all of you here, particularly to Dr. Ramey
11 and King, not just for the process here, but remember my
12 joke about Machiavelli and my quote about Machiavelli
13 and about what happens if you actually comment on things
14 where, you know, you try and get into actually what's
15 really the facts, clumsy truth. I think we should all
16 acknowledge that there are forces that might prevent us
17 from perhaps engaging in these sorts of issues and that
18 would be a shame. And if we choose not to engage in
19 issues which are of public interest, and I think we
20 should commend all those who do choose to engage in
21 issues in public interest like this and we should leave
22 it like that.

23 Congratulations to all of you. I
24 appreciate the effort you-all put into this, and I
25 hope that you will continue to do this. So thank you

1 all. That's it. We're done.

2 WHEREUPON, the within proceedings were
3 concluded at the approximate hour of 1:16 p.m. on the
4 7th day of July, 2006.

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REPORTER'S CERTIFICATE

STATE OF COLORADO)
) ss.
CITY AND COUNTY OF DENVER)

I, LYNNETTE L. COPENHAVER, Certified Shorthand Reporter and Notary Public, State of Colorado, do hereby certify that the said proceedings were taken in machine shorthand by me at the time and place aforesaid and was thereafter reduced to typewritten form; that the foregoing is a true transcript of the questions asked, testimony given, and proceedings had.

I further certify that I am not employed by, related to, nor of counsel for any of the parties herein, nor otherwise interested in the outcome of this litigation.

IN WITNESS WHEREOF, I have affixed my signature this 21st day of July, 2006.

My commission expires April 26, 2010.

_____ Reading and Signing was requested.

_____ Reading and Signing was waived.

__X__ Reading and Signing is not required.